Review #1

Response to general comments.

We thank the reviewer for this detailed review. We aim to address all points raised by adding clarification, expanding the introduction and discussion and providing an alternative representation for Figure 1. The reviewer requests that we separate results and discussion sections; on this point our preference is to retain the format as presented. 'Biogeosciences' welcomes both formats (combined or separate results and discussion sections). We do not feel that the argument for separating these sections (to '*help expert readers identify patterns and compare rates'*) should in any way be hindered by the choice of format. We are open to guidance from the Editor on this issue and will accommodate accordingly. We provide detailed responses below.

>Details of action taken in revised manuscript: We provide specific details below regarding the additional clarification we provide, and the points we have expanded upon. We have provided a revised version of Figure 1. We have chosen not to separate results and discussion sections as no preference or guidance was provided by the Editor on this point.

> *The reference to previous studies and interpretation of the data in the light of the planktonic community responsible for the fluxes measured is insufficient. Despite the authors only have larger phytoplankton counts and no flow cytometry data, their fluxes could be better discuss in the light of previous works in the NW African upwelling.*

We are willing to expand the discussion and citations related to the NW African region and thank the reviewer for this information. We would however suggest that the factors and topics critical to the discussion of our results have been included. Although we did not include it, we have analytical flow cytometry (AFC) data. However, this information was not especially insightful, partly due to the limited duration of the study. Using all parameter sets available to us, we undertook statistical analysis of this data in an attempt to understand links between physical, biological and chemical characteristics of the water column. Unfortunately, limitations (e.g. the duration of the filament study) precluded any statistically significant interpretation.

>Details of action taken in revised manuscript: We have expanded upon these points throughout the introduction and discussion sections, including a comment on our AFC analysis.

> *Given the existing literature on this subject published in the last 20 years, and the different approaches taken to measure new and regenerated production, it is perhaps the time to produce a review paper comparing the data obtained in different systems, homogenize results, and even provide the community with a consensus formulation for new and regenerated production (concepts which need to be revisited, as the authors discuss "it is likely that NO3 based exportable production from such systems has been over-estimated historically").*

This would be a valuable exercise, time permitting. We have gathered an extensive data set of N-cycle parameters across a range of oceanographic provinces; the process of synthesising this data into a review structure has started and we are very much interested in taking this further. We would welcome input from this reviewer should they wish to contribute.

>Details of action taken in revised manuscript: A comment – no action required.

ABSTRACT

The abstract contains many details on the data, but none on their interpretation.

The abstract provides an overview of the study objectives, the location, the method and the data obtained. The last sentence provides an insight into the implications of the study. We believe that this adequately fore fills the function of an abstract and do not propose to change it.

>Details of action taken in revised manuscript: No action taken.

INTRODUCTION

The introduction could highlight better how considering regenerated production may change our view of upwelling ecosystems and global carbon and nitrogen cycling in marine systems.

Page 17785, line 5: please also mention all other nitrogen fluxes that may influence f-ratio calculations (DON release, N2 fixation. . .), including appropriate references.

We will improve the consideration of regenerated nitrogen in the introduction and include N-cycle processes in the introduction of f-ratio calculations. We would not wish to cover this extensively in the introduction as it is covered further in the discussion, and elsewhere in the literature.

>Details of action taken in revised manuscript: The introduction now includes a fuller consideration of N-cycling processes relevant to themanuscript.

MATERIALS AND METHODS

Page 17785, line 20: please provide details on the remote sense data (satellite used etc), how much time passed between detecting the filament and starting sampling it

We have added this detail.

>Details of action taken in revised manuscript: This detail has been added to section 2.

Page 17785, line 25: "GC" is not described elsewhere in the text. More details on the SF6/3He detection method should be provided (i.e. are these compounds detected in a continuous way, like with seawater pump continuously through the GC? Or sampled discretely?

These details have been added.

>Details of action taken in revised manuscript: This detail has been added to section 2.0

Page 17786, line 1: when is that? Provide detection ranges, limits.

This information has been added

>Details of action taken in revised manuscript: Additional information has been added. As with the hydrographical survey work, the reader is referred to full details in the respective papers (Meunier et

al. 2012; Nightingale et al., 2000) as these are extremely complex areas of science and cannot be summarised in a few sentences.

Page 17786, lines 5-6: it would be clearer to list which stations were sampled at which depth levels and which were not.

Stations sampled at 1% sPAR are identified in Fig. 6; this only relates to NO_2^- oxidation rate data at 6 stations. Our view is that an additional table is not justified, but this information will be clarified in the text.

>Details of action taken in revised manuscript: In section 2.0 we have clarified which samples were taken at which depths; at 55% sPAR samples were taken for N-assimilation and regeneration studies. At 1% sPAR, only 6 samples were taken for NO2 oxidation.

Page 17786, line 12: do the authors know what's the temperature range within their flushing boxes along incubations, and/or how does it compare to in situ temperature?

Incubation boxes were flushed with seawater using the ships surface seawater supply, consistent with standard practice which was established over 3 decades ago (Glibert 1982, Mar. Biol. 70:209-222; Benavides et al 2014, J. Mar. Sys. 140:123-129; Varela et al 2003, J. Plank. Res. 25:719-736). The ships seawater supply (to both labs and decks) was collected from a depth of approximately 5m. We did not measure the temperature of individual incubation boxes. However, very high flow rates were used for this flushing process and we would not anticipate large deviations between surface seawater temperature and that of incubation boxes.

>Details of action taken in revised manuscript: None. As stated, this approach is guided by JGOFS protocols.

Page 17786, line 25: the authors mention chemicals and where they were acquired from before actually saying what they used them for. I would recommend describing how 15N additions were made and then stating in parentheses where the chemicals were obtained from.

This is essentially a point about writing style. Rather than be extremely repetitive (as many chemicals were sourced from 'Sigma-Aldrich' for example) we chose to gather this information together for collective presentation. We do not propose to change this.

>Details of action taken in revised manuscript: None.

Page 17786, line 27: why was the sample blacked-out? Was it only maintained in the dark until being dispensed in different incubation bottles and amended with 15N? Why regeneration fluxes were measured in the dark and assimilation ones in the light? Do the authors have any evidence that nitrogen regeneration is not performed by photo-heterotrophic bacteria for example?

The collection of seawater and 15-N amendments were conducted with blacked out containers to avoid light shock (some samples were collected from low light environments). However, the text is quite clear that all incubations were performed in deck incubators using simulated light and temperature according to JGOFS protocols. At no point is the use of 'dark' incubations introduced.

>Details of action taken in revised manuscript: We have added text to make it absolutely clear that seawater was collected and manipulated using blacked-out containers, but that incubations were conducted during day-light.

Page 17787, line 9: so one bottle or set of bottles was used to measure NH4 regeneration in the light and another one in the dark? It is unclear. Bottles were incubated in monoplicates but then split in triplicate subsamples for analysis? Are the error estimates given in Figure 5 analytical standard deviation? It is not clear from the methods.

The text in line 9 (P17787) states;

'Amended seawater was used to fill a 2.2 L incubation bottle, which was placed in a deck incubator at simulated light and temperature for approximately 8 h.'

This seems quite clear – a single 2.2L bottle is filled with 15-N amended seawater and placed in an incubator under simulated light and temperature. We are open to suggestions as to how this can be made any clearer. The remaining amended seawater (i.e. 4L minus the 2.2L used for incubation) is filtered and triplicate 100mL volumes used for pre-incubation analysis. Similarly, the text states that post-incubation, the 2.2L bottle contents are filtered and split into triplicate samples for post-incubation analysis. Again, we are open to suggestions as to how improve clarity here. We can improve clarity in the figures by stating what error bars represent; one standard deviation of triplicate rate estimations for Fig 5, or one standard deviation for triplicate concentration measurements for Fig 2 for example. We will add this clarification. However, to re-iterate, at no point do we discuss the use of 'dark' incubations and it is unclear how the reviewer has reached this interpretation.

>Details of action taken in revised manuscript: By addressing the previous points we believe that the issues raised here have been addressed. We add additional clarity by providing an indication of the time at which incubations were initiated and terminated; this underscores the point that these incubations were conducted during day-light.

Page 17787, line 18 and elsewhere: the range of %15N enrichment caused by 15Nlabeled substrate addition should be mentioned somewhere in the methods.

Indeed. This was an oversight and will be stated in the methods section.

>Details of action taken in revised manuscript: This information has been included.

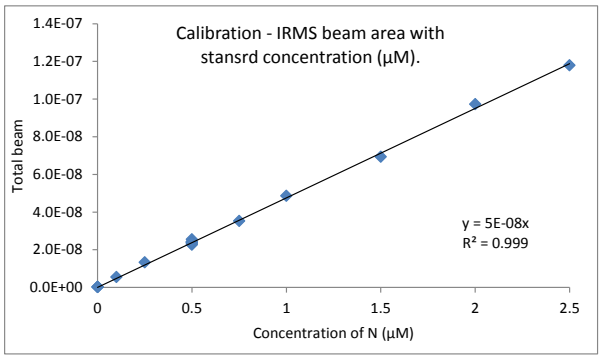
Pages 17788-17789: consider reducing methods and referring to previous publications where possible (see general comments above).

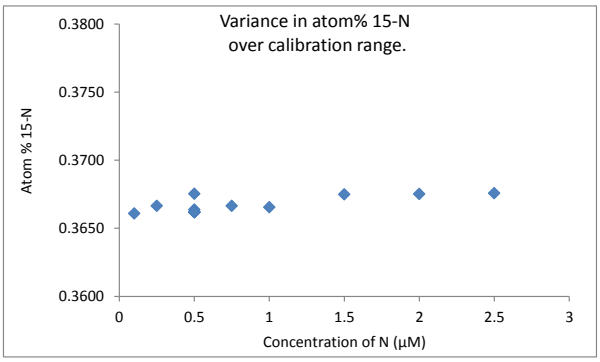
We find that this is a difficult balance – some readers (and reviewers) prefer to have full method details while others prefer very concise descriptions. We have made reference to our previous work for method details which are not provided here.

>Details of action taken in revised manuscript: We have opted not to modify the level of detail provided in the methods section. We have provided details of recent developments in the method which is not provided elsewhere. However, we have also cited our previous work for addition method details where appropriate.

Page 17790, lines 4 and following: 660 mL for 15NO3 and 15NH4 incubations seems a really small volume. What was the range of PON concentrations measured? What is linearity limit of the IRMS used? Can 15N values be given with enough accuracy which such low PON concentrations (if they were low)?

The volume and duration of incubations for N-assimilation measurements was absolutely appropriate for high productivity systems. The range of PON concentrations was presented in Fig 5 (panel b) and reported as 0.6-3.0µM-N. A calibration for our IRMS is presented below, in addition to variance in atom% 15-N for this standard range. The correlation provides an r^2 of 0.999 with a variance in atom%15-N of these standards of 0.16%. This is normal for such systems. Sample sizes <0.5µM-N are not used. Enrichments achieved during this study were as high as 1.0 atom%15N, and generally >0.5atom%15N. Consequently, analytical performance was not an issue during this study.





>Details of action taken in revised manuscript: there should be no question of isotope ratio mass spectrometry system performance at these PON concentrations as they are well within system capabilities. This would be readily recognised by independent users of such systems. We have not modified our text to address points raised here.

Page 17790, line 9: assimilation incubations were performed for 6 h, while regeneration ones were 8 h. The use of short incubations for the measurement of regeneration fluxes is common, but the assimilation incubation times seem short to me. The authors should state why these timings were chosen, as well as discuss how this might have affected their rates and compare to other studies using different incubation times.

We disagree. Quite the reverse in fact. Assimilation studies are often kept short; historically the rationale for this was that it minimised both the influence of isotope dilution (in the dissolved

fraction) due to N-regeneration and the risk of substrate depletion. In high productivity systems, incubations as short as 3 hours have been reported. This constrains atom%15N enrichment of PON thus avoiding analytical complications with excessive enrichment. Conversely, analytical (and method) limitations have often required long incubations for N-regeneration studies, as these rates are typically (though not always) lower than assimilation rates. The incubation durations used in our study were appropriate to the study location and are consistent with previous studies. We do not believe that justification is necessary; comparisons with other studies on this point would detract from the manuscripts focus and would be of little value.

>Details of action taken in revised manuscript: Our incubations were 6 hours for nitrogen assimilation and 8 hours for nitrogen regeneration studies. In support of this, we outline below examples of ^{15}N incubation durations from the literature, which includes other major upwelling systems, to demonstrate that these incubations times are completely consistent with standard practice.

Further, in the seminal paper of Harrison et al (1987; J. Plank. Res. 9:235-248, p236) the need for short incubation times for assimilation studies had already been recognised;

'Data sets and Methods. Eight data sets were analysed……..restricted analysis to….(ii) ^{15}N tracer incubations were short (h)…..The former restriction was applied because of our desire to extrapolate the derived relationship to the open ocean, where ammonium concentrations are known to be extremely low…and the latter because of recent concern over methodological problems related to long incubation times….'

On the separate point raised by the reviewer, a comparison between studies and incubation times is beyond the scope of this contribution and would be unlikely to provide useful insights due to the high degree of variability in biogeochemical context between studies. The only way to meaningfully address the influence of incubation time would be to conduct such studies on the same water sample which clearly we did not do (nor has been done by others to the best of our knowledge).

We have not modified the manuscript text as this point does not merit development.

Author	Process	Location and time
Glibert, 1982. Mar. Biol. 70:209-222.	NH_4^+ regeneration and assimilation.	Oceanic, coastal and estuarine. 1-4 hours for both processes
Benavides et al 2014. J. Mar. Sys. 140:123-129	NH_4^+ and NO_3^- regeneration and assimilation.	Namibian upwelling Up to 4 hours
Fernandez and Farías, 2012 MEPS, 451:1-14	NH_4^+ and NO_3^- regeneration and assimilation.	Chilean upwelling system 8-12 hours
Varela et al 2003 J. Plank. Res. 25:719-736	NH_4^+ assimilation and regeneration	Iberian upwelling, NW Spain 2 hours for both processes
Smith et al 2014 PLOS One 9(9):e108173	Nitrification (i.e. regeneration)	Monterey Bay upwelling 24 hours
Bode et al 2004	NH_4^+ regeneration (i.e. regeneration)	Iberian Upwelling, NW Spain

J Plank. Res. 26:11-28		2-3 hours.
Newell et al 2013 L&O 58:1491-1500	Nitrification (i.e. regeneration)	Sargasso Sea 12 hours

Page 17790, lines 10 and following: it is not clear how many replicates were done.

The text states on line 6 (p17790);

'Triplicate 660mL volumes of seawater were separately amended……'

i.e. there were 3 replicates.

>Details of action taken in revised manuscript: No change to the text as it is quite clear as presented.

Page 17791, line 9: this notation (T-1-T7) has not been explained before in the methods section.

Correct – this notation was 'introduced' later on page 17793. We will address this sequence.

>Details of action taken in revised manuscript: This notation is now introduced in section 2.0.

Page 17791, lines 15-20: a table including the variability of RNO3 values along the study would be helpful. So, in summary, the proportion of RNO3 with respect to total NO3 increases with distance to the recently upwelled water near the coast? Please add a conclusive sentence at the end of this paragraph.

The reviewers text is confusing here. 'RNO3 increases with distance 'TO' the recently upwelled water near the coast'. From where then?

The term 'R_{NO3}' represents the fraction of the total NO_3^- pool represented by 'new' NO_3^-. Upwelled water must, by convention at least, represent new NO_3^- alone. This fraction diminished as photic zone nitrification diluted the 'new' NO_3^- pool with 'regenerated' NO_3^-. That is the concept we are introducing here and representing by Fig. 7. RNO3 values are presented in Fig 7 and an additional table is not justified.

>Details of action taken in revised manuscript: Again, the reviewer's interpretation of this text is extremely confusing. Variability in RNO3, which relates to f-ratio values, is presented in Fig 7 and does not need to be presented again in a separate table. Perhaps the reviewer overlooked this important figure? We have not added or modified manuscript text in response to this confusing comment.

Page 17792, line 10: It is a pity that flow cytometry data are not available.

It is available. However the data adds little to the interpretation of results presented and so was not included in the manuscript.

>Details of action taken in revised manuscript: We have highlighted that AFC data was available but did not add to our understanding. We note that co-variance between N-regeneration and bacterial abundance is not evident and that other routes of NH_4^+ regeneration were likely to be of greater importance.

RESULTS AND DISCUSSION

At the time of their sampling the authors found that the filament contained mainly NACW, which has a lower nutrient content than SACW. Can the authors discuss, in the light of water mass depiction in this upwelling system (previous publications) how different water mass proportions and seasonality may affect microbial nitrogen fluxes on a yearly basis?

Unfortunately, the simple answer is no. We attempted to draw out some statistically meaningful interpretations between nitrogen fluxes and water column characteristics but were unable to do so due to data limitations, both spatially and temporally. While other studies can add to the data set, this type of analysis is beyond the scope of the study.

>Details of action taken in revised manuscript: We estimate annual new production from this upwelling region which goes some way to addressing this point. However, to specifically test the influence of different water masses on nitrogen cycle fluxes goes well beyond the scope of the study, which was to assess the significance of nitrogen regeneration for new production estimates in an upwelled filament. As with all such studies we can only work within the confines of the data we have – what the reviewer is requesting here is practically impossible to address.

Despite the authors do not have prokaryote abundance data, it is worth discussing their role in regeneration processes referring to previous works. Also, the use of GF/F filters and the related potential underestimation of nitrogen fluxes should be discussed (Morán et al., 1999).

We do have AFC data but it adds little value. We will expand the discussion of the role of prokaryotes in N-regeneration processes. The paper referred to by the reviewer relates to the estimation of primary production using ^{14}C and its overestimation using GF/F filters due to $DO^{14}C$ adsorption. This is a completely separate issue to the use of GF/F for PON retention and ^{15}N analysis. The only potential issue for N-flux (i.e. N-assimilation) rate estimations would be the underestimation of PON due to non-quantitative retention of all cells capable of utilising enriched inorganic nitrogen (i.e. the bacteria). This is unlikely to be a significant issue given that productivity in this system was dominated by large cells (> 2µm, Fig 4).

>Details of action taken in revised manuscript: We have expanded the introduction and discussion sections to more fully consider microbial N-cycling processes.

Page 17793, lines 26-27: the description of T-1 etc should be included in the methods section.

This issue has been addressed.

>Details of action taken in revised manuscript: This information is now introduced in section 2.0.

Page 17793, lines 18-20: it is unclear how the mapping was done. Regular (multiple) CTD casts? Moving vessel profilers, SeaSoar? It should be better explained in the methods.

Extensive details of the mapping exercise are provided in a highly technical form by Meunier et al. (2012), and referred to in the manuscript. While some additional details can be provided here, this is a complex area and interested readers should refer to the associated paper from this research program.

>Details of action taken in revised manuscript: Additional details have been added in section 2.0. However, we re-iterate that a study of the physical structure and associated hydrological mapping undertaken during this program was presented in the Meunier et al. (2012) paper which we cite.

Page 17793, lines 25-27: so the Lagrangian study was successful from days 0 to 7, but not beyond (lines 5-7), or is it not fully reliable from days 0 to 7?

This is a confusing interpretation of the manuscripts text. We will add text to clarify the constraints of the study duration; these details have already been requested by the reviewer in a previous comment.

>Details of action taken in revised manuscript: We have added additional detail in section 2.0 which adds clarification.

Page 17794, line 17: Even if N2 fixation was not targeted in this study (and is likely minimal in this upwelling system) it should be mentioned and discussed. The low N:P ratios found could promote diazotrophic activity. It is worth discussing works like Raimbault and Garcia (2008), Sohm et al. (2011) and others. The subject is briefly mentioned in Page 17795, line 6, but could be further developed.

We have attempted to focus the text on topics directly relevant (and informed) by the data. Deviation in N:P is directly relevant for subsequent N-fixation as the water mass leaves the study area and advects offshore. We discuss this. Noting that we are not offering a review, without further data we question the value of discussing N-fixation in any depth as we are unable to offer anything other than speculation.

>Details of action taken in revised manuscript: In the introduction (section 1) section we have added information about N-fixation and its potential contribution to new nitrogen inputs to the study region. In the discussion (section 3.1) we expand the relevant information we have to the N-fixation debate regarding N:P.

Page 17794, line 27: you mean denitrification?

No. We mean nitrification as stated. Denitrification is a strictly anaerobic process, most often associated with sediments. We are referring to pelagic nitrification, which is well documented in oxygen minimum zones (OMZ). A by-product of the nitrification process is N_2O. Consequently, pelagic OMZ's are a source of N_2O.

>Details of action taken in revised manuscript: We have added citations to support the position that denitrification is not a process that can be considered to explain the observations as it is associated with anoxia whereas our measurements take place in an oxygenated environment.

Page 17795, line 2: the authors mention here P without having explained what it is, how it is calculated etc.*

We have included this detail.

>Details of action taken in revised manuscript: We have included this information in section 3.1

Page 17795, line 2: It is probably out of the scope of this paper, but can the authors use offshore transversal velocities to estimate how much phosphate (and other nutrients) are exported offshore (export rates) by the filament and compare with other upwelling systems? It would be interesting to compare with data from other filaments like those in California, off the Iberian Peninsula and Cape Ghir, for example. Presumably physical data can be easily obtained from Meunier et al. 2012 (?). This could be included at the end of the discussion (Page 17802).

This is an interesting point and we had considered how to arrive at an estimation of P-export (specifically) as this would be most relevant to the basin scale P^* debate. However, the complexity of this issue justifies a separate manuscript and is beyond the scope of the present contribution.

>Details of action taken in revised manuscript: No action taken as it is beyond the scope of the manuscript.

Page 17795, line 8: very vague, please be more precise.

Is the reviewer referring to the statement '….biological processes…' in relation to the drawdown of inorganic nutrients? We could add that the processes relate to primary production, but this really is a statement of the obvious. Perhaps the reviewer is referring to the second part of this sentence; '…horizontal and vertical mixing' but again, in the context of the preceding sentence the meaning should be clear? We do not propose to modify this sentence as we do not agree that its meaning is vague.

>Details of action taken in revised manuscript: No action taken. The reviewer is not clear about which aspect of the text is perceived as vague. Our view is that this text is clear and defer to the Editor for guidance.

Page 17795, line 12: add references to figures where needed.

Additional figure references will be added as needed.

>Details of action taken in revised manuscript: Figure references have been added where appropriate.

Page 17795, lines 13-21: and why is this? Different nutrient regime? Try to compare with other upwelling systems (reasons behind different primary production rates).

There are far too many possible reasons behind this to make meaningful comparisons within the constraints of available space. Such information has been reviewed and links to nutrient regimes and seasonality have been made. For individual studies, productivity could also depend upon the 'age' of upwelled water (i.e. since it reached the photic zone and supported photosynthetic processes) which cannot necessarily be known. We do not propose to develop this point as it would be highly speculative.

>Details of action taken in revised manuscript: The reviewer is essentially asking; 'What influences the productivity of different upwelling regimes?' This is a highly complex subject area and a topic of continued debate. It has been reviewed and discussed previously (e.g. Chavez and Messié, 2009, Progress in Oceanography 83:80–96). Given the limited nature of the data set in terms of space/time, we are unable to fully address such a complex question.

Page 17795, lines 22-23: state ranges, refer to figure.

This information has been included.

>Details of action taken in revised manuscript: Text describing ranges and figures included in section 3.1.

Page 17796, lines 9-10: state ranges of phytoplankton abundance (here and in the following lines).

This information has been included.

>Details of action taken in revised manuscript: We add some additional information to this text in section 3.2. However, the actual abundance data is presented in Fig 5 to which the reader is referred.

Page 17796, line 11: at least state % of carbon provided by diatoms.

This information has been included.

>Details of action taken in revised manuscript: We add some additional information to this text in section 3.2

Page 17796, line 15: the high proportion of flagellates is important and explains the predominance of regeneration fluxes. Discuss further, cite other works where the protagonism of flagellates in upwelling systems has been highlighted (e.g. Anabalón et al., 2014; Böttjer and Morales, 2007).

There is a coincidence between peak NH_4^+ regeneration rates and peak flagellate abundance but one does not necessarily explain the other. We discuss the routes of NH_4^+ regeneration (17798 line 14-20) and support this with numerous citations. Within the constraints of the data we believe this is adequate (we will be including more information about regeneration processes in the introduction).

>Details of action taken in revised manuscript: We have added a more in depth discussion of the role of the microbial community in nitrogen regeneration (section 3.3) and speculate that there is a link between observed rates of NH4 regeneration and flagellate abundance, as suggested by the reviewer.

Page 17796, line 17: POC would be better if available.

It isn't.

>Details of action taken in revised manuscript: None.

Page 17796, lines 20-24: See Benavides et al. (2013).

We will include appropriate information from this publication.

>Details of action taken in revised manuscript: Information from this citation has been included.

Page 17798, line 17: or active release.

Indeed. We will provide any appropriate support to this statement.

>Details of action taken in revised manuscript: The introduction and discussion of nitrogen regeneration has been expanded.

Page 17798, line 20: and probably negligible in your samples due to the small volume used.

We suspect that the reviewer is referring to lines 18-19 (not line 20) which describes as minor the contribution from zooplankton activity to NH_4^+ regeneration. The sample volume will have been likely to exclude zooplankton and so their contribution to NH_4^+ regeneration would have been negligible. However, we believe that the text already makes this point adequately.

>Details of action taken in revised manuscript: None as this is a comment.

Page 17800, lines 4-24: before speculating on particle-attached nitrifying organisms, I suggest discussing the much more oxygenated character of this upwelling system in comparison with Peru for example and how this affects nitrification/denitrification processes.

Firstly, there is evidence for particle attached nitrifying organisms presented in the citation (Ward 2008). Secondly, denitrification is not taking place in the incubation bottles used here and cannot be considered as a mechanism to explain these observations. Yes, denitrification takes place in systems like the Peruvian upwelling, but how does this help to explain the observations made here, in aerobic water samples taken from the photic zone? The discussion suggested by the reviewer would be entirely speculative. By contrast, the proposed mechanism aligns with the observations (e.g. DIN budgets) and provides a rationale for related observations (such as the challenge of linking pelagic nitrification rates with environmental drivers). Finally, we have direct (but yet unpublished) observational data from NERC's (UK) Shelf Seas Biogeochemistry (http://www.uk-ssb.org/) program that supports the association between very high rates of N-regeneration and marine particles; N-regeneration rates vary in relation to particle composition, depth and season.

>Details of action taken in revised manuscript: We have not modified this text as the speculation we present is supported by as yet unpublished empirical evidence and is consistent with observations presented here and elsewhere (e.g. Clark et al 2011).

Page 17800, lines 25 and following: here the authors could refer to the RNO3 table proposed above.

The RNO_3 data is already presented in a figure and does not need to be replicated in a table.

>Details of action taken in revised manuscript: None. The data is already presented clearly in a figure.

Page 17802: Have the authors tried converting new production to carbon using Redfield (or C:N ratios from their own samples) and comparing those rates to 14C-based primary production data?

The rationale for this exercise is not clear.

>Details of action taken in revised manuscript: None. Our export estimation combined a 'new-nitrogen' estimation with ^{14}C-based measurements of primary production. It is not clear how elemental stoichiometry (particulate or dissolved, neither of which we measured) comes into this as an alternative means of estimating export, which presumably is what the reviewer is suggesting.

Page 17802, conclusions: Can the authors estimate how much new production rates in upwelling systems are overestimated by not discerning between 'new' and 'regenerated' NO3?

We do this! P17802 line 5-10. We present annual new production estimated by the classical f-ratio and then present how much this decreases by with each 'correction' strategy.

>Details of action taken in revised manuscript: We are concerned by this comment. These estimations are an important outcome of the study and are described in section 3.5. it is not clear how the reviewer missed this information.

Figure 1: This figure needs to be improved. The longitude is not aligned between panels. The path of the labeled water mass (SF6 distribution) should be superimposed on the map. Satellite images of Chl and/or temperature would be helpful for the reader to see the structure of the upwelling filament.

We will develop a new figure using a combination of satellite data and regional maps.

>Details of action taken in revised manuscript: This figure has been completely re-done.

Figure 3: This figure is barely discussed in the text. The differences in N:P ratios between MLD and below MLD are interesting and merit discussion. Also, MLD is not written in full at first use in the text (Page 17798, line 4).

We will define MLD and expand the discussion of this figure. The data demonstrates the concept of P export (i.e. P*) by this filament. As suggested by the reviewer, this point could be expanded upon to arrive at a useable number for this export flux. We will also speculate (to a limited extent) upon the difference between values between depths.

>Details of action taken in revised manuscript: MLD is defined on first use (section 3.0). We expand the discussion of this figure in section 3.1.

TECHNICAL CORRECTIONS

Lagrangian and Eulerian should be capitalized throughout the text.

Page 17786, line 17: CTD is fully written here, but in fact was mentioned before in the text (line 3 of the same page).

We will address these points.

>Details of action taken in revised manuscript: This has been done

Page 17794, line 28: here the authors use 'N' instead of 'nitrogen', please be consistent throughout the text. Same in Page 17796, line 1 and elsewhere.

We will correct this for consistency.

>Details of action taken in revised manuscript: This has been done

Page 17795, line 24: 'northwest African', also written as 'North West', and 'NW' elsewhere in the text. Please be consistent.

We will correct this for consistency.

>Details of action taken in revised manuscript: The term 'North West' has been used throughout (with the exception of references where the title is presented as it appears in the publication).

Page 17800, line 3: typo "µmnol"

Well spotted. We will address this point.

>Details of action taken in revised manuscript: This has been addressed.

Review #2

We thank the reviewer for this thoughtful review.

Specific comments

1) NH4+ regeneration: There might be NH4+ production by photochemical processes as well, see e.g., Rain-Franco et al. (2014). So, I am wondering whether NH4+ regeneration by photoproduction in the upwelling off Mauritania/NW Africa may play a role as well.

>This is a good point and would be likely to contribute to the process in situ. As noted by Rain-Franco et al (2014), the processes of photoproduction and NH_4^+ regeneration cannot be separated. We will acknowledge that a contribution from photoproduction is a possibility in our data set although we are unable to estimate its contribution. It is also worth noting that by using polycarbonate incubation bottles we exclude any contribution to photoproduction of NH_4^+ from UV.

>Details of action taken in revised manuscript: We have added text to the introduction and discussion (section 3.3) to highlight the possibility that photo-production of NH_4^+ may have contributed to NH_4^+ regeneration rates measured.

2) N deposition by aerosols may play a role for new production too; especially in view of the fact that filaments off NW Africa can receive a lot of Saharan dust input. Please discuss.

>Reviewer 1 requested that we expand our consideration of N-sources that contribute to f-ratio determinations. Atmospheric dust inputs of nitrogen falls within this scope and will be included in the revised version of the manuscript.

>Details of action taken in revised manuscript: The introduction (section 1) now includes this aspect.

3) Nowald et al (2015) present particle flux (OM flux) data from a sediment trap deployed at the same time (and very close to the filament track) of the study described in the ms under review. I am wondering whether the OM flux data by Nowald et al may match those presented in Section 3.5.

>While the Nowald et al (2015) paper is interesting, it is difficult to directly link our studies. Nowald et al compare seasonal patterns of particle fluxes using sediment traps deployed at 1100 meters. The authors note that we do not fully understand the processes which influence particle formation, transport and destruction and links between surface ocean chlorophyll and sediment trap records are poorly understood. However they also note that once particles escape the zone of biological activity little subsequent transformation takes place. Consequently, there are indirect links between our studies in that new production (C-export; our study) estimates set an upper limit for vertical C-flux (i.e. the Nowald study). We will cite the Nowald et al paper and highlight this indirect link although we do not propose to make direct comparisons between our data sets.

>Details of action taken in revised manuscript: We have cited the paper in section 3.1 and included a statement which links our studies.

4) There are rather old (but nevertheless important) studies on nutrient distribution and primary production off Mauritania/NW Africa by Minas et al. (1982a, b; 1986) which are ignored. Minas et al. calculated f ratio (0.9), N:Si ratios and measured PP rates. I suggest that these data are included in the discussion.
>Unfortunately we do not have access to the Minas et al 1982a publication. However, Minas et al 1986 refer to a separate study (Minas et al 1982) in which an f-ratio was estimated in the absence of NH_4^+ regeneration data; a value of 0.9 was provided and deemed to be an over-estimate. Their revised value of 0.64 in Minas et al 1986 is almost identical to that measured on the first day of this study (0.61-0.63, depending upon method used to account for N-regeneration). We will include this comparison in our discussion.

>Minas et al (1986) provide an estimation of primary production of 2.312 g Cm^{-2} d^{-1} for the NW African upwelling. This value falls within the range we report from our study and will be included in our discussion.

>Details of action taken in revised manuscript: The primary production estimate by Minas et al 1986 has been included in section 3.1. f-ratio values provided by Minas et al 1982; 1986 have been included in section 3.4

5)In Zindler et al. (2010) N:P ratios and phytoplankton composition from the upwelling off Mauritania are presented. This ref. should be cited as well (see e.g., Sections 3.1 and 3.2).
>Zindler et al (2010) presented N:P values approaching 20 for freshly upwelled water, dropping to 0.1 as upwelled water advected offshore. This supports our suggestion that we caught an early stage of the upwelling process, but possibly not the earliest stage (values in the upper MLD were 13:1). We will include this point and cite the Zindler et al paper.

>Details of action taken in revised manuscript: This text has been added in section 3.1.

6) p. 17800: I am not fully convinced by the discussion about particle associated nitrification. In a recent study by Ganesh et al (2014) it was shown that indeed denitrification is particle associated but not nitrification. So, I suggest that denitrification in sinking particles could take place in oxic subsurface water masses off NW Africa.
>The literature on microbial activity in association with particles is complex. Ganesh et al (2015; p2687) state that while sequences matching ammonia and nitrite oxidising microbes were more abundant in free living rather than particle bound (1.6 – 30μm) fractions, they were nevertheless identified in both fractions suggesting that nitrifying organisms were particle bound in this OMZ (noting that the study was of an OMZ rather than oxygenated water column, which modifies the activity of N-cycle microbes). We acknowledge that this suggestion is speculative (we are unable to present our direct evidence relating to the European Shelf Sea) and perhaps this point needs to be

emphasized. However, the mechanism we propose is consistent with our observations. It offers an explanation as to how the decoupling between NH_4^+ and NO_2^- oxidation (observed in many studies as cited) can be sustained.
> We make the point that denitrification is unlikely to be taking place in our incubations of an aerobic water column (we report O_2 concentrations); we will add that this is because denitrification is associated with low oxygen or anaerobic conditions, in contrast to the conditions of our study.
>In our revised manuscript we will emphasise that this proposed mechanism is speculative. We can offer no more than the reasoned arguments already presented.

>Details of action taken in revised manuscript: In section 3.3, we have incorporated the Ganesh et al 2015 citation. We have highlighted the fact that denitrification is a processes associated with anoxic conditions, in contrast to our experimental conditions and added citations to support this.